A Comparison of Conjoint Analysis Response Formats

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A split-sample design is used to evaluate the convergent validity of three response formats used in conjoint analysis experiments. We investigate whether recoding rating data to rankings and choose-one formats, and recoding ranking data to choose one, result in structural models and welfare estimates that are statistically indistinguishable from estimates based on ranking or choose-one questions. Our results indicate that convergent validity of ratings, ranks, and choose one is not established. In addition, we find that people frequently use "ties" in responses to rating questions, and that the option not to choose any of the alternatives ("opt-out") affects some preference estimates.

Key words: conjoint analysis, convergent validity, forest valuation, hicksian surplus.

Critiques of contingent valuation in the early 1990s (Hausman NOAA Panel) appear to have accelerated research efforts to investigate the use of conjoint analysis and choice-based methods for eliciting nonmarket values (Adamowicz, Louviere, and Williams; Adamowicz et al.; Garrod and Willis; Johnson and Desvousges; Johnston and Swallow; Holmes, Zinkhan, and Mercer; Layton and Lee; Mackenzie; Opaluch et al.; Roe, Boyle, and Teisl; Stevens, Barrett, and Wills). This is a logical progression as these methodologies are all "close cousins" in the general family of stated-preference methods. In fact, they are so closely related that it would be surprising if any one approach proved to be a panacea for all of the problems that have been asserted to apply to stated-preference methods. However, cross fertilization of these literatures is likely to have a positive influence in the further refinement of stated-preference methods applied to the elicitation of nonmarket values.

A review of these methodologies reveals an extensive literature dedicated to investigating the validity and reliability of Hicksian surplus estimates derived from contingent valuation (Hanemann Mitchell and Carson). While conjoint analysis has an extensive literature (Louviere, 1988a and 1988b), applications to the estimation of Hicksian surplus are rather new. Thus, it makes sense that some of the same issues that have been investigated for contingent valuation should also be investigated for other stated-preference methodologies, including conjoint analysis. For example, while we know that contingent-valuation response formats (e.g., open-ended, payment card, and dichotomous choice) influence estimates of Hicksian surplus (Welsh and Poe), we do not know how different response formats in conjoint studies (e.g., ratings, ranks, and choose one) influence estimates of Hicksian surplus. A related issue is that contingent-valuation questions ask respondents to reveal information about their Hicksian surplus directly, while conjoint studies ask people to reveal relative preference orderings. If the choices do not include a "would not buy" or "status quo" alternative, a nonzero value is implied in the estimated likelihood function for people who would not choose one of the alternatives. In general, this serves to bias estimates of Hicksian surplus upward.

In this study, we investigate whether recoding ratings to ranks or choose one, and recoding ranks to choose one, result in comparable

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The authors thank Shelley Phillips-Mills and Genevieve Pullis for their research assistance, and Malcom Hunter and Alan Kimball for their help in designing the survey instrument. The authors also thank George Peterson, Tom Brown, and David Layton for their helpful suggestions.

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structural models and welfare estimates as would occur if respondents simply answered a ranking or a choose-one question. We do this with a split-sample design where respondents are randomly assigned to answer either a rating, a ranking, or a choice question. Estimation results for ratings recoded to ranks are compared with estimation results for the rank responses, and estimation results for ratings and ranks recoded to choose one are compared with the estimation results for the choose-one responses. This is a test of convergent validity, a la the Welsh and Poe comparison of contingent-valuation response formats, which asks whether different methods of estimating a theoretical construct provide similar estimates (Carmines and Zeller). If preferences are truly transitive and the three response formats do not induce different methodological effects, it is expected that null hypotheses of no difference could not be rejected. If either of these conditions is violated (transitivity or response format neutrality), it is likely that the null hypotheses will be rejected. However, rejection of the null hypotheses will not tell us whether the differences are due to intransitive preferences, response formats that are not neutral, or both of these issues.

In conducting conjoint and choice experiments, some investigators have included a "do nothing" alternative, while others have not. For example, Adamowicz, Louviere, and Williams and Adamowicz et al. allowed respondents to indicate that they would not participate in a recreational-demand setting (no choice). In applications valuing public programs, investigators allowed one of the alternative commodities to be the status quo (Johnston and Swallow; Johnston et al.; Layton and Lee; Roe, Boyle, and Teisl). Johnson and Desvousges specified one of the alternatives randomly provided to respondents as the status quo, but the status quo was not assigned to all respondents in all choices. Other investigators do not appear to have included a no-choice or status quo alternative in their designs (Holmes, Zinkhan, and Mercer; Mackenzie; Opaluch et al.).

These mixed signals in the literature led us to design a two-stage question to study preferences over timber-harvesting practices in Maine.[1] In the first question, we asked respondents to rate, rank, or choose one of four alternative commodities (i.e., practices) where the status quo was within the alternatives from which the four alternatives were randomly assigned. This is similar to the Johnson and Desvousges study where some, but not all, respondents evaluate the status quo. Given our concern of whether respondents were "in the market," we followed the conjoint questions with a set of four single-bounded, dichotomous-choice questions to see if respondents would "buy" any of the four alternatives. People who answered no to all four questions would be left with the status quo.[2]

Previous Research

Although several empirical studies focused on comparing the degree to which conjoint response formats affect estimation results, our search of the literature did not uncover any single study that compares rating, rank, and choose-one response formats using independent samples. Krosnick and Alwin find that ratings and ranks both lead to similar aggregate measures of predicted choices, although they are dissimilar with respect to the underlying estimated structure of preferences (see also Russell and Gray). The rating responses include a group of individuals who rate alternatives equally (ties). Removing these "nondifferentiators" from the data produced results similar to those derived from the rank data. Kalish and Nelson find little difference in the performance of ratings and ranks, particularly in predicting choice in a hold out sample of individuals. In comparing ratings and choose-one data, Louviere and Gaeth find that these approaches produce similar aggregate utility functions, while Elrod, Louviere, and Davey find that these approaches work equally well at predicting choice in a hold out sample. Louviere, Fox, and Moore compare ratings and choice data and find that both approaches produce coefficient estimates that are equivalent up to a rescaling.

[1] In our application the status quo represents the bundle of timber-harvesting practices that is believed by experts to represent those in most common usage. In fact, all of the attribute levels included in our study are used in Maine to a greater or lesser extent.

[2] Our study differs from the Adamowicz et al. studies in that we were not valuing a recreational activity where respondents could choose not to recreate. In our study, rejection of all of the alternatives meant that respondents were left with the status quo, similar to the Johnson and Desvousges, Johnston and Swallow, and Johnston et al. studies. We analyze the effect of with and without the no-choice alternative in another paper. Here, we simply use these data to test the sensitivity of the with and without the no-choice alternative on the comparisons of rating, ranking, and choose-one response formats.

Rating responses to conjoint questions have been criticized for their use in welfare analyses due to their cardinal properties. In response to these criticisms, some researchers have appealed to the transitivity of preferences and recoded ratings to ordinal ranks (Layton and Lee; Mackenzie; Roe, Boyle, and Teisl; Stevens, Barrett, and Willis). Mackenzie also advocated the use of ratings based on a presumption that they provide more statistical information than ranks and choices. For example, two commodities, defined in terms of different attribute levels, could be rated on a scale from 1 (most desirable) to 6 (least desirable). Recoding to ranks assigns the commodity with the higher rating a rank of "1" and the commodity with the lower rating a rank of "2." Likewise, when recoding the ratings to choose one, the commodity with the higher rating is assigned as the chosen alternative. Roe, Boyle, and Teisl find that ratings recoded to choose one produce the tightest confidence intervals around estimates of compensating variation. Furthermore, they find that ratings recoded to ranks produce the largest welfare estimates followed by ratings and the ratings recoded to choose one. Stevens, Barrett, and Willis, in comparing ratings and choice data (where the choices are recoded from ratings), also find that choice data produce the smallest welfare estimates.

A potential problem with all of these studies, except for Kalish and Nelson, is that within-sample comparisons were conducted. Some studies have participants answer more than one response format, while other studies have participants answer one response format and the response data are recoded to simulate data from another response format. Within-sample comparisons are problematic because when differences are detected in responses to different conjoint questions it is impossible to determine whether the differences are due to the conjoint response formats or due to a question-ordering effect.

Conceptual Framework

In analyzing each of the three types of response data, we assume that respondents have linear preferences over the forest attributes in the experimental design such that

$$(1) \qquad V(\cdot) = \boldsymbol{\beta}\mathbf{A} + \alpha\$ + e$$

where $V(\cdot)$ is an indirect utility function, $\boldsymbol{\beta}$ is a row vector of coefficients, $\mathbf{A}$ is a column vector of forest attributes, α is the marginal utility of money, \$ is the monetary incentive included in the experimental design, and e is the random econometric error. Measures of compensating variation are then

$$(2) \qquad CV_{i-j} = \mathbf{b}(\mathbf{A}_i - \mathbf{A}_j)/a$$

where a and b denote estimated coefficients for (α and $\boldsymbol{\beta}$, respectively, and i, j denote different levels of attributes. The 90% confidence intervals for welfare estimates are derived by bootstrapping from the original data. For each model, N observations are randomly sampled with replacement from the original data set of size N. The model coefficients are estimated from the resampled data and the compensating variation measure is calculated from these coefficients. This procedure is repeated 1000 times for each model. The fifth and ninety-fifth percentiles of these distributions define 90% confidence intervals.

Data Analyses

We analyze the ratings data using a double-hurdle tobit model and an ordered-probit model. Both models are motivated by a postulated continuous latent variable that is partially observed. The double-hurdle tobit model treats the ratings as a cardinal measure of utility that is censored at both ends of the rating scale. This approach requires an assumed transformation from ratings space to utility space, and carries the implicit assumption that the utility distance between each unit change in ratings is constant (Roe, Boyle, and Teisl). In the ordered-probit model, the rankings have no cardinal interpretation, i.e., a rating of three is not necessarily twice as far from a rating of one as a rating of two.

Rank data are analyzed with an ordered-probit model and, following Garrod and Willis and Layton and Lee, these data are also analyzed using a rank-ordered logit model. Rank-order estimation exploits all rank information by implicitly assuming that each rank is made as part of a sequential random utility selection process. The alternative ranked first is assumed to have been chosen because it yields higher utility than the other three alternatives. It is assumed that respondents repeat this random utility maximization with the remaining three commodities. Another datum is created to represent the fact that the second-ranked alternative is chosen over the other two alternatives. A last datum represents the fact that the third-ranked alternative is chosen over the alternative ranked last. All three data points (or more generally, $K-1$ data points derived from K commodity ranks) are used to estimate the rank-order logit model.

The choose-one data are analyzed using a probit model. This model includes four observations per respondent; with the chosen alternative coded as "1" and the other three alternatives coded as "0."

Data Recoding

If respondent preferences are transitive across conjoint response formats, respondents put in the effort to reveal their preferences fully with each response format (Holmes, Zinkhan, and Mercer), the different response formats are neutral in the elicitation of preferences, then ranks should be recoverable from rating data and choose-one choices should be recoverable from rating or ranking data. That is, the recoded rating data and the original rank data can be analyzed using the same models. A likelihood-ratio test can be conducted to investigate the null hypothesis:

$$(3)\quad H_O\text{: } \boldsymbol{\beta}_{RT\rightarrow RK} = \boldsymbol{\beta}_{RK} \text{ versus } H_a\text{: not } H_O$$

where $\boldsymbol{\beta}_{RT\rightarrow RK}$ is the vector of parameters for the rating data recoded to ranks and $\boldsymbol{\beta}_{RK}$ is the vector of parameters for the ranking data. Similar tests can be conducted for the parameters estimated using ratings and ranks recoded to choose one versus the parameters for the choose-one data.

To explore the effects of ties in the rating data (Mackenzie; Layton and Lee; Roe, Boyle, and Teisl; Stevens, Barrett, and Willis) several ordered-probit models are estimated that use recoded rating data that either *exclude* or *include* all observations with tied ratings. When ties are included, an ordered-probit model is estimated where ties are coded to the highest rank, e.g., a ratings sequence of 1, 3, 3, 5 (lower numbers being better) is coded to ranks as 1, 2, 2, 4.

A censored, rank-order logit model, as used by Layton and Lee, is used to analyze the recoded ratings data that include ties. Censored, rank-order logit accommodates instances where only *partial orderings* of the data are available, *as is the case of ties* in the ratings data recoded to ranks. This modeling framework allows the researcher to avoid making the subjective decision of whether tied ratings should be assigned the higher (as done here) or lower rank (see Layton and Lee). Ties in ratings recoded to ranks are handled by exploding the ranks only when ties do not exist. For example, if A, B, C, and D are rated as 1, 2, 2, and 6 (lower numbers being better), a censored, rank-order logit observation is based on three datum: A chosen from A, B, C, and D; B chosen from B and D; and C chosen from C and D. Just as in the case of a complete ranking, three data points are derived. However, less information is used in estimation because complete ranks present a choice from four alternatives, a choice from three alternatives, and a choice from two alternatives. The censored, rank-order logit, as presented in this example, is based on a choice from four alternatives and two choices from sets of two alternatives.

To explore recoding ratings and ranks to choices, probit models were estimated for rating and rank data converted to choose-one data. For the rating data, probit models were estimated for recodings that *exclude* all observations with ties and those that *include* all observations with ties.

To maintain consistency of the distribution assumption, all statistical tests were conducted using the probit models. The tobit, rank-ordered logit, and censored, rank-ordered logit models are included to consider how different modeling frameworks affect welfare estimates for the rating and ranking data, respectively.

Survey Procedures

Preferences of Maine residents for timber-harvesting practices were solicited using a mail survey instrument.[3] Respondents were asked to consider seven timber-harvesting attributes (table 1). Information regarding the forest practices was presented by enclos-

[3] Three studies attempted to investigate nonmarket values for forest practices in Europe (Garrod and Willis; Hanley and Ruffell; Mattsson and Li). These studies indicate that the public is able to distinguish between different forest management practices, that positive welfare estimates exist for more environmentally benign approaches to forest management (Mattsson and Li), and that substantial nonuse values can be generated by management practices that promote an increase in biodiversity (Garrod and Willis).

Table 1. Forest Practices and Levels

Practices	Levels
Road density	One road every mile **One road every half mile**
Dead and dying trees left standing in harvest openings	**Remove all** Leave one dead or dying tree about every 93 feet (Five trees per acre) Leave one dead or dying tree about every 66 feet (ten trees per acre)
Live trees left standing in harvest openings	No trees greater than 6 inches left standing **One tree 6-inches thick about every 17 feet (153 trees per acre)** One tree 6-inches thick about every 5 feet (459 trees per acre)
Maximum size of harvest openings	Less than 5 acres **5–35 acres** 36–125 acres
Percentage of land available for timber harvesting	**80% for timber harvesting and 20% as a natural area** 50% for timber harvesting and 50% as a natural area 20% for timber harvesting and 80% as a natural area
Size of watershed protection zones	At least a 500-foot zone **At least a 250-foot zone**
Slash disposal (bark, branches, and stumps left after harvesting)	Leave where it falls on the ground Distribute along skid trails **Remove all**

Note: Levels of harvesting practices believed to be in most common usage are denoted in bold.

ing an information booklet that accompanied the questionnaire.[4]

Within the questionnaire, a scenario for evaluating the forest practices described the State purchasing a 23,000-acre parcel of forest land. The forest practices described how timber harvesting is to be managed on this land. Respondents were told the land will be purchased from a large forest, land-management company, were given a brief description of the parcel, and were provided with a map that identified the general geographic location of the parcel.

Respondents were then presented with four management plans to consider. Each management plan was composed of randomly assigned levels of each of the forest practices identified in table 1.[5] The practices are listed in table 1 in the order presented to respondents. An eighth attribute was included, which is a one-time increase in state income taxes to pay for the proposed purchase of public forest land. We also examine the implied compensating variation of moving from the status quo forest practices to more environmentally benign timber harvesting (table 2).

The design of the questionnaires for each of the three subsamples was identical except for the conjoint question. The rating question asked:

> How would you rate the desirability to you of each of the proposed forest management plans for this piece of forest land?

Respondents provided ratings on an integer scale that ranged from 0 (very undesirable) to 6 (very desirable). The rank question asked:

> How would you rank the desirability to you of each of the proposed forest management plans for this piece of forest land with one (1) being *most* desirable and 4 being *least* desirable?

[4] The information booklet and the survey instrument were pretested in focus groups held in Bangor ($n = 5$) and in Portland ($n = 7$); the two largest communities in Maine.

[5] Many researchers use orthogonal designs whereby the combinations of the practices–levels are reduced to an independent, parsimonious group. This implies that the effects of the practices are linearly additive. We did not wish to impose this assumption since there may be combinations of the attributes that respondents find to be particularly undesirable or particularly desirable. We do not investigate this issue here, but it is certainly an issue of concern for future research initiatives.

Table 2. Variable Levels for Computation of Marginal Compensating Variation

Variable	Current Harvesting Practice	More Benign Harvesting Practices
ROADS	0 = 1/2 mile	1 = 1 mile
DEAD5	0 = none	1 = 5/acre
DEAD10	0 = none	0 = otherwise
LIVE153	1 = 153/acre	0 = otherwise
LIVE459	0 = otherwise	1 = 459/acre
HOPEN35	1 = 5–35 acres	1 = 5–35 acres
HOPEN125	0 = otherwise	0 = otherwise
H20ZONE500	0 = 250 feet	0 = 250 feet
PERH50	0 = otherwise	1 = 50%
PERH80	1 = 80%	0 = otherwise
REMSLASHASH	1 = remove	0 = otherwise
DISTSLASH	0 = otherwise	1 = distribute

Respondents filled in a blank for each of the four management plans with the ranks (1, 2, 3, and 4). The choose-one question asked:

> Which of the proposed forest management plans for this piece of forest land is the most desirable to you?

Respondents circled the letter of the management plan they would choose. The followup, dichotomous-choice question asked:

> Now assume there will be a vote in the next election to decide if the State of Maine should buy this piece of forest land. How would you vote for the purchase if the land were managed using each of the proposed forest management plans?

Respondents indicated how they would vote for each of the four forest management plans they were asked to consider.

Sample

The sample was composed of 2500 randomly selected individuals, eighteen years of age or older, from records of Maine drivers licenses and state identification cards. Each of three subsamples (n = 697, n = 730, n = 743) received one of the response formats for the conjoint question.

The survey was administered by mail in early 1997. Of the initial sample of 2500 individuals, 451 had addresses that were undeliverable by the U.S. Post Office (18%). A total of 926 surveys were completed and returned for a usable response rate of 45%. The number of returned surveys by version is 334 for ratings, 297 for ranks, and 295 for choose one. The usable response rates by version are 48% for ratings, 45% for ranks, and 42% for choose one. The percentages of attribute levels in the response data closely correspond to those in the original design. Thus, effort and response time, as investigated by Holmes, Zinkhan, and Mercer, does not appear to have resulted in respondents self-selecting on the response formats or the attribute levels.

Results

The most telling result arises from the response distributions. In the ratings data, different people provided ratings with one, two, three, or four ties in their responses. In stark contrast, people answering the ranking question did not use ties. This is also true for the choose-one responses. We did not tell respondents they could or could not use ties in answering any of the three response formats. Thus, respondents obviously felt free to use ties in answering the rating question, but must have felt compelled to distinguish between alternatives for the rank and choice questions.[6] This finding suggests that the different response formats are not neutral in the elicitation of preferences. If we cannot establish convergent validity of the rating with

[6] This result is similar to that of Alwin and Krosnick, who had ties in their rank data. We assume that these researchers, like us, did not give respondents any direction with respect to the use of ties (see also Alwin and Krosnick).

ranks or choices, methodological effects arising from the use of ties in the ranks is a likely culprit. Thus, differences could be solely due to methodological effects and not due to intransitivity of preferences. The issue of ties, however, does not apply to the comparison of recoded ranks with choose-one responses.

Base Model Results

The tobit and ordered-probit analyses of the rating data result in the same variables being significant and the signs of these significant variables are the same in both equations (table 3). It is interesting that the tobit and ordered-probit analyses of the ratings data result in compensating variation estimates that only differ by $41 (which is less than 5% of the smaller estimate). These results indicate that the presumed cardinality of the rating data in the tobit analysis is not an issue in the current application.

The ordered-probit and rank-ordered logit analyses of the ranking data result in all of the same variables being significant with one exception (PERH50), and the signs of all significant variables are the same in both equations. These analyses of the ranking data show a greater difference in welfare estimates than was found in the rating data, ($286, which is less than 25% of the lower estimate).

The ordered-probit analyses of the rating and rank data and the probit analysis of the choose-one data show mixed results in terms of the significance and signs of the estimated coefficients. ROADS and PERH80 are only significant in the rating and rank models. DEAD5 and PERH50 are only significant in the rank and choose-one models. Leaving a 500-foot wetland protection zone (H20ZONE500) is only significant in the rating model. Only PERH50 has a sign reversal between equations for variables with significant coefficients in at least one model; it is positive in the choose-one model and negative in the other models. The mean compensating-variation estimates for a change from the status quo to more benign harvesting practices, as defined in table 2, range from $1,603 (ordered-probit model of ranks) to $957 (choose one), bounding the welfare estimate for the ordered-probit model of the ratings. These results, like the respondents use of ties, provides additional suggestive evidence that the three conjoint response formats are not neutral in the elicitation of preferences.

Converting Ratings to Ranks

The ordered-probit models of the rating data and the rank data are replicated in table 4 for purposes of comparison. Recoded rating data that excludes ties results in a smaller number of significant variables relative to the ordered-probit analysis of the rating data. This result is not purely an artifact of recoding, as the sample size has been substantially reduced by the elimination of ties. The ordered-probit analysis of the recoded data with ties also results in differences in significant variables relative to the ordered-probit analysis of the rating data; two variables become significant (DEAD5 and HOPEN35) and one variable becomes insignificant (ROADS). In addition, the welfare estimates for the models excluding and including ties are lower than welfare estimates for the original ordered-probit analysis of the rating data. Note that the welfare estimate for the ordered-probit model with ties is approximately the same as that of the censored, rank-ordered logit model ($980 versus $984).

A likelihood-ratio test for the null hypotheses that the vectors of coefficients for the recoded ratings models, analyzed with an ordered-probit model, are the same as the ordered-probit model of the rank data cannot be rejected at the 10% level ($\chi^2(16) = 23.54$) for the model with no ties ($\chi^2 = 14.82$) and can be rejected for the model with all of the data ($\chi^2 = 134.96$). This result is similar to that of Alwin and Krosnik, but one must ask whether the insignificant test result is due to a loss in power from the reduction in the number of observations when ties are excluded. The ordered-probit analysis of the recoded ratings yields a much lower welfare estimate than the ordered-probit analysis of the ranking data ($980 versus $1,603). The analyses of the recoded ratings data also give lower welfare estimates than the analysis of the rating data ($980 versus $1,356).

Converting Ratings and Ranks to Choose One

The ordered-probit models of the rating and ranking data and the choose-one probit model are replicated in table 5 for purposes of comparison. The models using ratings (ties included) and ranks recoded to choose one show a number of differences relative to the choose-one model. ROADS,

Table 3. Base Models for Data Analyses

	Ratings		Ranks		Choose One
Variable	Tobit	Ordered Probit	Ordered Probit	Rank-Ordered Logit	Probit
INTERCPT	1.9775*[a]	0.6092*	−0.9208*	NA	−1.0154*
	(0.3211)[b]	(0.1209)	(0.1476)		(0.1557)
ROADS	0.4278*	0.1587*	0.1557*	0.2124*	0.0294
(1=1/mile)	(0.1693)	(0.0624)[b]	(0.0756)	(0.1049)	(0.0868)
DEAD5	0.2971	0.1160	0.3056*	0.3867*	0.3099*
(1=5/acre)	(0.2078)	(0.0766)	(0.0928)	(0.1329)	(0.1069)
DEAD10	0.5364*	0.1984*	0.2776*	0.4212*	0.4424*
(1=10/acre)	(0.2107)	(0.0777)	(0.0918)	(0.1332)	(0.1067)
LIVE153	0.8029*	0.3198*	0.3950*	0.5151*	0.4325*
(1=153/acre)	(0.2059)	0.0762	(0.0917)	(0.1274)	(0.1079)
LIVE459	0.6370*	0.2504*	0.3539*	0.04407*	0.3677*
(1=459/acre)	(0.2041)	(0.0754)	(0.0926)	(0.1284)	(0.1053)
HOPEN35	0.2346	0.0949	0.0966	0.1504	0.0088
(1=5–35 acres)	(0.2060)	(0.0759)	(0.0925)	(0.1282)	(0.1048)
HOPEN125	−0.1806	−0.0595	0.0872	0.0629	0.0261
(1=35–125 acres)	(0.2073)	(0.0764)	(0.0909)	(0.1290)	(0.1054)
H2OZONE500	0.4067*	0.1495*	0.0519	0.0608	−0.0075
(1=500)	(0.1688)	(0.0622)	(0.0751)	(0.1045)	(0.0860)
PERH50	−0.0742	−0.0264	−0.1732*	−0.1940	0.2374*
(1=50% harv.)	(0.2084)	(0.0768)	(0.0936)	(0.1285)	(0.1036)
PERH80	−0.6853*	−0.2608*	−0.3573*	−0.4703*	−0.0910
(1=80% harv.)	(0.2037)	(0.0752)	(0.0919)	(0.1296)	(0.1068)
REMSLASHASH	−0.3851*	−0.1547*	−0.2757*	−0.3512*	−0.2014*
(1=Remove all)	(0.2081)	(0.0767)	(0.0913)	(0.1300)	(0.1066)
DISTSLASH	−0.0864	−0.0372	0.0374	0.0154	−0.0719
(1=distribute)	(0.2057)	(0.0758)	(0.0923)	(0.1312)	(0.1036)
TAX	−0.0012*	−0.0004*	−0.0006*	−0.0009*	−0.0008*
($)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
SCALE	2.7354*				
	(0.0762)				
INTER.2		−0.3676*	0.7298*		
		(0.0296)	(0.0445)		
INTER.3		−0.8020*	1.4522*		
		(0.039)	(0.0578)		
INTER.4		−1.0641*			
		(0.0436)			
INTER.5		−1.5466*			
		(0.0506)			
INTER.6		−2.1432*			
		(0.0618)			
Mean *CV*[c]	$1,315	$1,356	$1,603	$1,317	$957
	[648, 2231]	[718, 2244]	[981, 2389]	[749, 2078]	[451, 1562]
N[d]	1148	1148	856	639	1112

[a] Asterisks denote significance at the 10% level.
[b] Standard errors are reported in parentheses.
[c] Compensating variation estimates for a change from the status quo to more benign harvesting practices as denoted in table 2, with 95% confidence intervals in brackets.
[d] Number of conjoint responses used in the estimation.

Table 4. Models for Ratings Converted to Ranks

	Ratings	Rating Converted to Ranks			Ranks	
		Ordered Probit		Censored		Rank,
Variable	Ordered Probit	No Ties	All Ties	Rank-Order Logit	Ordered Probit	Ordered Logit
INTERCPT	0.6092*[a]	−0.5732*	−0.5050*	NA	−0.9208*	NA
	(0.1209)[b]	(0.2714)	(0.1260)		(0.1476)	
ROADS	0.1587*	0.02181	0.08140	0.1822	0.1557*	0.2124*
(1=1/mile)	(0.0624)	(0.1381)	(0.0658)	(0.1153)	(0.0756)	(0.1049)
DEAD5	0.1160	0.1646	0.1822*	0.4195*	0.30562*	0.3867*
(1=5/acre)	(0.0766)	(0.1663)	(0.0805)	(0.1427)	(0.0928)	(0.1329)
DEAD10	0.1984*	0.2160	0.2580*	0.5207*	0.2776*	0.4212*
(1=10/acre)	(0.0777)	(0.1771)	(0.0820)	(0.1410)	(0.0918)	(0.1332)
LIVE153	0.3198*	0.6889*	0.2897*	0.6592*	0.3950*	0.5151*
(1=153/acre)	0.0762	(0.1719)	(0.0803)	(0.1414)	(0.0917)	(0.1274)
LIVE459	0.2504*	0.1538	0.18644*	0.3963*	0.3539*	0.4407*
(1=459/acre)	(0.0754)	(0.1653)	(0.0792)	(0.1366)	(0.0926)	(0.1284)
HOPEN35	0.0949	0.2415	0.1741*	0.2416*	0.0966	0.1504
(1=5–35 acres)	(0.0759)	(0.1658)	(0.0805)	(0.1376)	(0.0925)	(0.1282)
HOPEN125	−0.0595	0.03436	0.03401	−0.0392	0.0872	0.0629
(1=35–125 acres)	(0.0764)	(0.1688)	(0.0803)	(0.1366)	(0.0909)	(0.1290)
H2OZONE500	0.1495*	0.0099*	0.1313*	0.1604	0.0519	0.0608
(1=500)	(0.0622)	(0.1360)	(0.0658)	(0.1130)	(0.0751)	(0.1045)
PERH50	−0.0264	−0.4117	−0.0436	−0.0041	−0.1732*	−0.1940
(1=50% harv.)	(0.0768)	(0.1682)	(0.0818)	(0.1375)	(0.0936)	(0.1285)
PERH80	−0.2608*	−0.4281*	−0.2169*	−0.4945*	−0.3573*	−0.4703*
(1=80% harv.)	(0.07518)	(0.1640)	(0.0791)	(0.1376)	(0.0919)	(0.1296)
REMSLASHASH	−0.1547*	−0.3680*	−0.2658*	−0.4216*	−0.2757*	−0.3512*
(1=Remove all)	(0.0767)	(0.1712)	(0.0811)	(0.1376)	(0.0913)	(0.1300)
DISTSLASH	−0.0372	−0.1903	−0.1261	−0.1939	0.03740	0.0154
(1=distribute)	(0.0758)	(0.1656)	(0.0805)	(0.1364)	(0.0923)	(0.1312)
TAX	−0.0004*	−0.0009*	−0.0005*	−0.0011*	−0.0006*	−0.0009*
($)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)
INTER.2	−0.3676*	0.7645*	0.4828*		0.7298*	
	(0.0296)	(0.0825)	(0.0308)		(0.0445)	
INTER.3	−0.8020*	1.5494*	1.7404*		1.4522*	
	(0.0395)	(0.1086)	(0.0587)		(0.0578)	
INTER.4	−1.0641*					
	(0.0436)					
INTER.5	−1.5466*					
	(0.0506)					
INTER.6	−2.1432*					
	(0.0618)					
$\chi^2 : (\boldsymbol{\beta}_{RT \to RK} = \boldsymbol{\beta}_{RK})^c$	NA	14.82	134.96*	NA	NA	NA
Mean $CV(\$)^d$	$1,356	−$170	$980	$984	$1,603	$1,317
	[718, 2244]	[-898, 443]	[416, 1813]	[420, 1612]	[981, 2389]	[749, 2078]
N^e	1148	272	1148	567	856	639

[a] Asterisks denote significance at the 10% level.
[b] Standard errors are reported in parentheses.
[c] Likelihood-ratio tests that coefficients for the models with recoded ratings ($RT \to RK$) are the same as models of the rank data (RK).
[d] Compensating variation estimates for a change from the status quo to more benign harvesting practices as denoted in table 2, with 95% confidence interval in brackets.
[e] Number of conjoint responses used in the estimation.

HOPEN35, and H20ZONE500 are only significant in the model with recoded ratings. DEAD5 and PERH50 are significant in the models with recoded ranks and choose one. DEAD10, LIVE 153, LIVE 459, and REMSLASH are significant in all three models. HOPEN125 is only significant in the model with recoded ranks. PERH80 is significant in the model with recoded rating and recoded ranks, and DISTSL is not significant in any of the three models.

Likelihood-ratio tests indicate that pairwise comparisons of the models with recoded rating and rank data versus the model of the original choose-one data result in the rejection of the null hypothesis of identical vectors of coefficients data at the 10% level ($\chi^2(14) = 21.06$) for recoded ratings with ties ($\chi^2 = 25.10$) and recoded ranks ($\chi^2 = 26.90$), but not for recoded ratings without ties ($\chi^2 = 18.52$). (Again, this may be due to the loss of statistical power.) The welfare estimates for the models with the recoded data are substantially larger than those from the choose-one data (\$1,618, \$1,712, and \$2,761 versus \$957).

Excluding People Who Would Not Choose Any of the Alternatives

Overall, only 8% of the sample would not choose one of the alternatives they were asked to evaluate.[7] We investigate the effects of excluding the people who would not choose any of the four alternative using the probit models.

We first ask whether the preferences of people who would choose at least one of the alternatives are different from those who would not choose any in terms of the estimated parameters in the probit models. That is,

$$(4) \qquad H_O : \boldsymbol{\beta}_{RT,1Y} = \boldsymbol{\beta}_{RT,0Y} \text{ versus } H_a : \text{ not } H_O$$

where RT refers to rate data and 1Y and 0Y imply at least one yes and zero yes responses, respectively, to the four dichotomous-choice questions. Similar tests are carried out for the rank and choose-one data. Significant differences occur at the 10% level for the rating ($\chi^2 = 148 > \chi^2(19) = 27$) and rank ($\chi^2 = 26 > \chi^2(16) = 24$) data, but not for the choose-one data ($\chi^2 = 17 < \chi^2(14) = 21$).

Next, we redo the comparisons of ratings converted to ranks, and ranks and ratings converted to choose one. In terms of converting ratings to ranks, we get the same pattern of statistical results as reported previously. When ties are excluded (no ties), the ordered-probit models for the recoded rating data and the rank data are not significantly different ($\chi^2 = 15.33 < \chi^2(16) = 23.54$) at the 10% level. The models are significantly different when all ties (four ties) are allowed ($\chi^2 = 102.62 > \chi^2(16) = 23.54$). Thus, the anomalous result for the no-tie comparison persists.

In the statistical comparisons of ratings and ranks recoded to choose one, no differences occur in the qualitative results. The comparison of ratings converted to choose one, when ties are excluded, is not significantly different at the 10% level ($\chi^2 = 20.95 < \chi^2(14) = 21.06$), and the models are different when all ties are allowed ($\chi^2 = 26.34 > \chi^2(14) = 21.06$). The comparison of the recoded rank data with the choose-one data also results in a significant difference ($\chi^2 = 28.10 > \chi^2(14) = 21.06$).

Thus, while the treatment of people who would not choose any of the four alternatives appears to affect preference estimates in terms of the estimated model coefficients, this issue does not appear to affect the comparisons of responses to rating, ranking, and choose-one questions conducted here. One key insight is that people only applied ties in answering the rating question and this methodological effect would not likely be affected by the inclusion or exclusion of a no-choice option.

Discussion

The raw distributions of responses suggest that differences will occur between ratings and ranks or choose-one estimation results; respondents only used ties in answering the rating question. Moreover, the results for the base models (table 3) suggest that the

[7] At first, it may appear somewhat surprising that only 8% of the sample would not choose any of the four alternatives. The reason for this, we believe, is that there is a general dissatisfaction with the status quo of forest management in Maine. There have been two state-wide referendums to change forest management practices and many bills before the state legislature have dealt with forestry issues in recent years. In addition, our alternative levels for each of the attributes generally presented the presumed status quo or more benign levels. Thus, respondents generally were evaluating alternatives that were more benign in terms of the undesirable effects of forest management practices.

Table 5. Models for Ratings and Ranks Converted to Choose One

	Ratings	Ratings Converted to Choose One		Ranks	Ranks Converted to Choose One	Choose One
		Probit				
Variable	Ordered Probit	No Ties	All Ties	Ordered Probit	Probit	Probit
INTERCPT	0.6092*[a]	−0.9981*	−0.7492*	−0.9208*	−1.0244*	−1.0154*
	(0.1209)[b]	(0.1948)	(0.1637)	(0.1476)	(0.1904)	(0.1557)
ROADS	0.1587*	0.2438*	0.1836*	0.1557*	0.0768	0.0294
(1=1/mile)	(0.0624)	(0.1017)	(0.0869)	(0.0756)	(0.0991)	(0.0868)
DEAD5	0.1160	0.3271*	0.1481	0.3056*	0.4294*	0.3099*
(1=5/acre)	(0.0766)	(0.1278)	(0.1072)	(0.0928)	(0.1240)	(0.1069)
DEAD10	0.1984*	0.3518*	0.2178*	0.2776*	0.3348*	0.4424*
(1=10/acre)	(0.0777)	(0.1290)	(0.1080)	(0.0918)	(0.1241)	(0.1067)
LIVE153	0.3198*	0.5010*	0.4423*	0.3950*	0.5191*	0.4325*
(1=153/acre)	(0.0762)	(0.1258)	(0.1071)	(0.0917)	(0.1234)	(0.1079)
LIVE459	0.2504*	0.4373*	0.3815*	0.3539*	0.4938*	0.3677*
(1=459/acre)	(0.0754)	(0.1242)	(0.1062)	(0.0926)	(0.1245)	(0.1053)
HOPEN35	0.09489	0.1858	0.1699*	0.0966	0.0602	0.0088
(1=5–35 acres)	(0.0759)	(0.1212)	(0.1042)	(0.0925)	(0.1222)	(0.1048)
HOPEN125	−0.0595	−0.0777	−0.0973	0.0872	0.1930*	0.0261
(1=35–125 acres)	(0.0764)	(0.1246)	(0.1074)	(0.0909)	(0.1184)	(0.1054)
H2OZONE500	0.1495*	0.1594	0.1994*	0.0519	0.0904	−0.0075
(1=500)	(0.0622)	(0.1005)	(0.0864)	(0.0751)	(0.0983)	(0.0860)
PERH50	−0.0264	−0.1028	0.0064	−0.1732	−0.2784*	0.2374*
(1=50% harv.)	(0.0768)	(0.1185)	(0.1034)	(0.0936)	(0.1194)	(0.1036)
PERH80	−0.2608*	−0.5755*	−0.4041*	−0.3573*	−0.4568*	−0.0910
(1=80% harv.)	(0.0752)	(0.1237)	(0.1057)	(0.0919)	(0.1192)	(0.1068)
REMSLASHASH	−0.1547*	−0.3212*	−0.3564*	−0.2757*	−0.4448*	−0.2014*
(1=Remove all)	(0.0767)	(0.1239)	(0.1070)	(0.0913)	(0.1230)	(0.1066)
DISTSLASH	−0.0372	−0.0995	−0.0960	0.0374	0.1050	−0.0719
(1=distribute)	(0.0758)	(0.1186)	(0.1029)	(0.0923)	(0.1158)	(0.1036)
TAX	−0.0004*	−0.0007*	−0.0006*	−0.0006*	−0.0004*	−0.0008*
($)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0001)	(0.0001)
INTER.2	−0.3676*			0.7298*		
	(0.0296)			(0.0445)		
INTER.3	−0.8020*			1.4522*		
	(0.0395)			(0.0578)		
INTER.4	−1.06419					
	(0.0436)					
INTER.5	−1.5466*					
	(0.0506)					
INTER.6	−2.1432*					
	(0.0618)					
$\chi^2 : (\boldsymbol{\beta}_{RT\to C1} = \boldsymbol{\beta}_{C1}, \boldsymbol{\beta}_{RK\to C1} = \boldsymbol{\beta}_{C1})^{c}$	NA	18.52	25.10*	NA	26.90*	NA
Mean CV^{d}	$1,356	$1,618	$1,712	$1,603	$2,761	$957
	[718, 2244]	[937, 2756]	[932, 2693]	[981, 1389]	[1471, 5579]	[451, 1562]
N^{e}	1148	836	1016	856	856	1112

[a] Asterisks denote significance at the 10% level.
[b] Standard errors are reported in parentheses.
[c] Likelihood-ratio tests that coefficients for the models with recoded ratings ($RT \to C1$) and recoded rankings ($RK \to C1$) are the same as the choose-one model ($C1$).
[d] Compensating variation estimates for a change from the status quo to more benign harvesting practices as denoted in table 2, with 95% confidence interval in brackets.
[e] Number of conjoint responses used in the estimation.

three response formats may each generate different statistical information regarding preferences. This suggestive evidence is confirmed when the likelihood-ratio tests are conducted. The parameters of the ordered-probit model using ratings recoded to ranks (ties allowed) is significantly different from the ordered-probit model of the original rank data. Likewise, the vectors of parameters from the probit models of ratings recoded to choose one (ties allowed) and ranks recoded to choose one are significantly different from the probit model of the choose-one data. The lack of statistical differences when ties are not allowed is similar to the finding of Krosnick and Alwin, but the perverse welfare estimate for ratings recoded to ranks and the large reduction in the number of observations suggests that the lack of significance may be a statistical anomaly.

The fact that people use ties in the ratings, but not the other responses formats, leads one to question whether the absence of ties in the ranks and choose-one data is reflective of respondents making more careful distinctions or making forced, arbitrary distinctions. The results of Ben-Akiva, Morikawa, and Shiroish; and Feather suggest that there may be problems with requiring forced ranks. This is an issue that deserves further consideration, even in studies that present one choice versus the status quo (Johnson and Desvousges; Johnston and Swallow) or two alternatives plus a no choice (Adamowicz, Louviere, and Williams and Adamowicz et al.) if respondents are asked to make multiple, repeated choices.

Our results are also qualitatively similar to those of Elrod, Louviere, and Davey and Kalish and Nelson. While the comparisons with ties result in different structural models as reflected by the test of parameter vectors, the confidence intervals on the welfare estimates overlap. We are concerned about the sizes of the confidence intervals, but using alternative procedures to calculate the confidence intervals results in similar or even larger confidence intervals. Using the Krinsky–Robb procedure resulted in a confidence interval for the choose-one estimate of (353, 1635) versus the interval of (451, 1562) reported in table 3.

Roe, Boyle, and Teisl and Stevens, Barrett, and Willis found that recoding ratings to choose-one data resulted in the smallest welfare estimates, where as in our study this recoding resulted in the largest welfare estimates (table 5). For our data the actual choose-one data resulted in the smallest welfare estimate (excluding the negative estimate for ratings recoded to ranks without ties). In addition, Roe, Boyle, and Teisl found that ratings recoded to ranks had the smallest confidence intervals, while here the ordered-probit analysis of the rank data has the tightest confidence interval around the mean (−39%, +49%).

Finally, it is worth noting that the differences in the welfare estimates from the rating, ranking and choose-one data, as reported in table 3, are actually smaller than Welsh and Poe found for contingent-valuation response formats. Welsh and Poe report estimated means of $37, $54, and $98, respectively, for payment-card, open-ended, and dichotomous-choice data. This is a range of $61 that is 165% of the smallest estimate. The comparable range for our data is $646, which is only 68% of the smallest estimate ($957 for choose one). It is also interesting to note that while dichotomous choice and choose one are the closest response formats conceptually and analytically, dichotomous choice provides the highest contingent-valuation welfare estimate and choose one provides the lowest conjoint welfare estimate. However, the 95% confidence interval on Welsh and Poe's dichotomous-choice estimate is (−16%, +26%), which is much smaller than the 95% confidence interval on our choose-one estimate (+53%, +63%).

Conclusions

Collectively, our results indicate that convergent validity of ratings, ranks, and choose one is not established. It is not advisable to recode ratings to ranks or choose one. The inconsistent use of ties (only in ratings) suggests that the problem may be methodological and not a violation of the transitivity of preferences. The comparison of ranks and choose one, however, is not complicated by the issue of ties and recoding ranks to choose one did not result in the same model as the actual choose-one data. The proposed statistical efficiency advocated by Mackenzie does not hold for estimates of Hicksian surplus. Rankings have the tightest confidence interval around the mean (−39%, +49%), while the confidence intervals for ratings (−47%,

+65%) and choose one (−53%, +63%) are roughly comparable.

Failure to establish convergent validity for the conjoint response formats investigated here is no different from what Welsh and Poe found for contingent valuation, and the range of the estimates is actually smaller. Which conjoint response format to use in future studies is not clear. The concerns regarding the implied cardinality of ratings and the fact that recoded ratings do not recover ranks or choose one would seem to eliminate rating. Choose one may be desirable because it avoids concerns of cardinality and provides the most conservative welfare estimate. Ranks and choose one, which is a special case of ranks, both warrant further investigation of the effect of forced choices without ties and inclusion–exclusion of a no-choice option on parameter estimates and on welfare estimates.

[Received May 1999; accepted June 2000.]

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